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Dear Dave:

We've been away and back, and in and out, and had other things pressing, so
I haven't gotten to your ms. on sequential gene transfer in time to be any
critical use to you, but here's how anyhow. What are your publications plans
on this?

$$\text{and preponderant}$$

Jacob has been making such a plausible/case for the sequence hypothesis
that I wonder ifxx if there is much point in my maintaining any cautionary
reserve on it any longer. It certainly doesn't seem like a bad idea, but I
only wish it were possible to make a unified theory that would include the
diploid data as well. It is not too hard to imagine that the characteristic
point of breakage is (at least often) postzygotic in effect, and that this is
superimposed on breakages from interrupted conjugations. The uniformity of the
diploids would then depend a) on the regularity of breakage proximal to Mal, and
b) marker-choice bias for nondisjunctions still heterozygous for the markers
proximal to the break. Or, instead of b), nondisjunction may not happen unless
most of the proximal region had participated in fertilization. I still do think
that an examination of nondisjunctional types might give the most direct evidence
for a variability of fertilization types.

As to the ms., I suppose you really will have to get most of your criticism
from Jacob. You have an accumulation of conjugants over 40+ minutes; he says
the mating is completed almost instantly.

It might help to understand the figure if you wrote the following equivalents:
(p = pair; z = zygotes):

   Malv = p+p.z   Lacv = p + p.z + z   Lacv.T6 = z + p.z

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   p. 2 Lac v as I used it implied a continuous variegation. I have used, and
would prefer Lacs for Lac-sectored. At the least, the distinction in behavior
upon replating should be emphasized.

   p.3 last paragraph: what is expected, yes or no?

   p.1 line 5: Do you attribute observations on exconjugants to Hayes[4]?

   p.6 Ref. 10 You really ought to quote Nelson and Lederberg 1954 on this.
There we did consider that elimination might be both pre- and postzygotic, but
thought (possibly wrongly) that this was superfluous. It still doesn't have
to be variable points of breakage; what you observe, is variable incidence of

"incorporation"— to give the most concrete model, stress on a chromosome region could influence pairing and crossing-over. Without pairing, the distal markers can't be recovered. That gene transfer may be normally partial is an economical working hypothesis, if you mean it ought to be sometimes partial. The generality of... complete transfer is open to question, but it looks to me as if there is evidence for both//.

Figure 2: Why are discontinuities a function of _time_ unless your plating procedure is disrupting pairs?

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                              to
We are very sorry not/have seen you at all this summer. The meeting schedules this year were almost criminal; I can't understand this craze to have these topics (and the audiences) beaten over the head so often. Baltimore and Ann Arbor were too much already; we ran away to Woods Hole for a couple of weeks.

Last year was rather brutally taken up in writing papers & moving troubles etc; I'm looking forward to seeing what a lab is like again. We are rather more comfortably set up in the remodelled lab now. I'm hoping to spend some time on protoplasts for transductional experiments, and to finish up studies on a series of new Hfr mutants we isolated last year. Alan Richter and I haven't forgotten about F and motility & will try to get that out of the way (for our mutual relief!) as soon as possible, Unfortunately, we have been on the verge of a clean proof of a _selective_ effect, but haven't quite made it yet. (Did you see Hirota and Kikkawa in Nature; I'm not quite sure whether the believe it; Alan did one run, quite unsuccessfully.)

What are your plans next year?

                                                      Yours sincerely

                        & best to Linda

                                                      Joshua Lederberg

P.S.   ?? reprints of binary mutability.....

PPS. I warned you it might happen: Marcus Rhoades had the story rather garbled, thinking it was a _specific_ interaction between two loci. (This may show in the Baltimore proceedings,)